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CONTENTS

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THE TREND OF THOUGHT IN PHYSICS[1]

THE retiring vice-president for Section B is in the position of an electron who two years ago was hurled from his normal state of repose into a new orbit in which, according to true classical laws, he does nothing, and is not expected to do anything, but from which he is not allowed to return without emitting his quantum. And so, I appear before you to-day to emit my quantum in the form of an address. I am not unmindful of the fact that the emission of quanta takes place according to laws which are restricted by no precedent, so, on this occasion, I feel free to wander as I please in the realms of speculation.

From the quietude of a quarter of a century ago, when it appeared to many that physics had exhausted itself and all that was worth discovering had been discovered, and when would-be Ph.D.'s went about like roaring lions seeking something to measure and finding nothing but the density of a gas or the viscosity of a solid, from a state where the line of demarkation between what we could hope to probe further and what was apparently beyond our reach forever was so sharp, from a state where the mechanism of what was known was so very clear and the mechanism of what was unknown was so very obscure, we have, in a few short years, evolved a situation in which nothing is very clear and nothing is very obscure, but in which we have found courage to peer into the innermost shrines of nature with a hope of comprehension of what we may there find. And what have we found? We have found as part of the same mechanism phenomena which take place according to our most primitive and naïve conceptions and phenomena which appear to violate some of our most cherished ideas as to how we think nature ought to work. We might have been serenely happy in a state of mind where electrodynamics remained untouched, and it was necessary to appeal to the occult to explain the atom; but, when the atom draws upon electrodynamics just sufficient to spoil it and yet leave us bound to it, we may be excused for a feeling of confusion and for a tendency to join in the cynicism of the wag who said that atomic theory is classical

[1] Address of the retiring vice-president and chairman of Section B—Physics—American Association for the Advancement of Science, Washington, December 30, 1924.

electrodynamics on Mondays and Wednesdays and quantum theory on Tuesdays and Fridays.

THE REASONABLENESS OF PHYSICAL HYPOTHESES

I suppose that if one should ask the theoretical physicist what he is trying to do, he would be fairly safe in replying that he is trying to understand the universe. And what does this understanding of the universe mean? It means different things to different people. To most physicists, however, it means the realization of a process of logical reasoning by which the phenomena of nature may be deduced as a consequence of certain hypotheses which are accepted without further inquiry. We are all familiar with the satisfaction which we feel when things are accounted for on the basis of certain hypotheses which we like. But an attempt to establish even the most plausible of these hypotheses on any basis other than pure hypothesis usually results in sorry failure. The average physicist would doubtless be much pleased if he could see some way by which gravitation could be transmitted as the result of strain in an elastic medium. If some philosopher should ask him what he means by an elastic medium, he may be wary and give a formal definition, but the thing which really gives him satisfaction about the medium will be the thought of a piece of elastic and the belief that the medium acts in a way somewhat analogous to that piece of elastic. If the philosopher should ask why the elastic pulls, he will reply, "Well, that is a matter of cohesion, but we believe that when the molecules of the elastic are separated from each other, they tend to come together again." "But," says the philosopher, "why do they do this?" "Well," replies the physicist, "although we speak crudely of their being separated, we really believe that they are embedded in a medium which has elastic properties, so that it resists their separation." "But what do you mean by the medium having elastic properties?" says the philosopher. "That it is in some way analogous to a piece of elastic," says the physicist. "But why does the elastic pull?" says the philosopher. And so we are back where we started.

Well, has nothing then been accomplished by our physicist's theory? Is he really as big a fool as he looks? I think not, for if he had realized his ambition in creating an elastic theory, he would at least have shown that this phenomenon, gravity, which he does not understand, acts in the same sort of way as that other phenomenon concerned with the elastic which he also does not understand although he thinks he does. There is at least a unification of ignorance in the matter. Psychologically the theory satisfied our physicist because he had become familiar with the elastic before he was old enough to think about it and be astonished at its power to pull; and he had arbitrarily accepted this pulling property as something which needed no explanation or at any rate needed much less explanation than most other things. And so we all have what I may call criteria for reasonableness, in terms of which we think. They are functions of the age in which we live, and of the phenomena which have been studied and made familiar to us by our immediate predecessors.

Three centuries ago, the astronomer Francesco Sizzi, arguing against Galileo's discovery of Jupiter's moons, spoke thus:[2]

> There are seven windows in the head, two nostrils, two eyes, two ears, and a mouth; so in the heavens there are two favorable stars, two unpropitious, two luminaries, and Mercury alone undecided and indifferent. From which and many other similar phenomena of nature, such as the seven metals, etc., which it were tedious to enumerate, we gather that the number of planets is necessarily seven.
>
> Moreover, the satellites are invisible to the naked eye, and therefore can have no influence upon the earth, and therefore would be useless, and therefore do not exist.
>
> Besides, the Jews and other ancient nations as well as modern Europeans have adopted the division of the week into seven days, and have named them from the seven planets; now if we increase the number of the planets this whole system falls to the ground.

Now, argue as we may, this man was probably no fool. He was doubtless a man of eminence in his day. His criteria for reasonableness were different from ours. And there is a certain element of truth in his statements. There *are* seven days in the week, seven openings in the head, and so forth, and if he had pushed his ideas far enough, he might even have predicted something about the periodic table. His hypothesis that all things which exist have some use is not at all a bad hypothesis, and would correlate many facts in biology. It would suggest to us that the laws of electrodynamics could not be expected to hold in the atom, for to build an atom in this way would be very bad since it could not be permanent. Of course, his criteria for reasonableness seem very naïve to us. He did not know as much as we do, and did not have so many things to explain. If he had, he might have joined us in saying:

> If we set a tuning fork into vibration by means of another vibrating fork placed at a distance, we can, if we wish, trace out the air motions by which the vibrations are transmitted. We can demonstrate that the energy is handed on through the medium. A similar thing can be demonstrated when a cork in one part of a pond is made to oscillate through the agency of a stone thrown into the pond at another place, or when an object

[2] Quoted from Sir Oliver Lodge's "Pioneers of Science."

at one end of a table is shaken by vibrations communicated at the other end. Hence, if things do not take place in this way when an electron falling upon the target of an X-ray tube causes the ejection of an electron somewhere else, all of this is meaningless.

The Factors which have Moulded Thought on the Lines of Classical Dynamics

I believe some bygone astronomer said that the planets moved in circles because the circle is the perfection of symmetry. Now that would not have been a bad law if it had only been right, and if it had been a little more explicit as to the relation between the sizes of the orbits and the speeds of the planets; for, after all, a circle is a very nice curve for a self-respecting planet to move in. However, the language of celestial mechanics speaks otherwise. It says that the planet moves in a circle, or rather in an ellipse, because it would really like to move in a straight line, but the sun prevents it. Now passing over for the moment the question of what is meant by a straight line, I think that we should make a philosophical error in supposing that a planet should prefer a straight line to any other curve if left to itself, whatever that might mean. The fact is that if we analyze the motion of the planet and pick out the acceleration, we find that it is always directed towards the sun, and that its magnitude depends only upon the distance from the sun, following, in fact, the well-known inverse square law, so that at great distances from the sun, the motions of the planets tend more and more towards the rectilinear constant velocity type. There are many other cases where the motion of a body is completely described by the statement that its acceleration is a function only of its position in relation to other bodies, as, for example, the case of a stone swinging around at the end of a piece of elastic. In the case of the stone and the elastic, it is necessary to attach, as a coefficient of the acceleration, a mass factor depending on the stone, in order that the quantity so obtained shall be determined entirely by the position. It is because things are such that in a large number of cases the mass times the acceleration is determined entirely by the position of the body that it has been convenient to give a name to that quantity, and another name to the function of the position to which it is vectorially equal. Nobody could have prevented our defining force as proportional to the acceleration had some other law been true, but nobody would have done it. The orientation of our thoughts, the adjustments of our criteria for reasonableness so that the motions of the planets and other bodies shall be understood by us in terms of those criteria, is one where we define force to be absent if the body is moving without acceleration. There is no question of rightness or wrongness. It is a matter of definition. Absence of acceleration represents the state of nothingness which the mind takes as its origin from which to elaborate its thoughts. That the situation is not even one inherent in our primitive instincts is obvious when we recall the difficulty experienced by beginners in physics in understanding why a heavy body can be kept moving with constant velocity without the application of a force. I have a good deal of sympathy with them. For, why the second time derivative of the motion rather than the first or the third or the fifth should be elevated to fundamental importance is a question whose answer can not be elaborated out of nothing. The state of mind to which our experiences have adapted us is one in which, if the body is at rest or moves with constant rectilinear velocity, we are content, no questions are asked. If it moves otherwise, we have decided to ask why, and we have been led to inquire particularly as to the acceleration, because that is the quantity which in most of the cases which have interested us in the past may be expressed readily in terms of the position of the bodies in relation to other bodies. The law is simple. Of course, it is a very interesting thing that it should be possible to express the matter in any manner which is simple; but, in pondering over the reason for the simplicity of nature's laws, it is well to remember that that simplicity is imparted largely because mankind, being unconsciously wise, has chosen to give names to those things which are simply related. Herein is the origin of the conservation of energy. For the fact that the mass accelerations of the bodies are functions only of their positions leads by direct analytical procedure to the result that the sum of the one half mv^2 plus a certain function of the positions of the bodies is always constant. Hence, the desirability of giving names (kinetic and potential energies) to the two quantities which possess such a simple and striking property. So much does the mind love the idea of something remaining constant that directly the laws of any new branch of physics are formulated, they are immediately searched for something which these laws keep constant in such a way as to permit of its being christened energy.

And so we have grown up in a frame of thought which speaks of forces, masses and accelerations. The law of action and reaction and its extension in the principle of d'Alembert merely place restrictions on the types of the forces which occur, and the equations of Lagrange, the principle of least action and the Hamiltonian principle merely form analytical elaborations of the scheme suitable for application to special types of problems.

THE DESCRIPTION OF NEW LAWS IN THE LANGUAGE OF THE OLD

When the accelerations are no longer functions of the position alone, but involve, for example, the velocities, we retain the form of the language with the stipulation that the forces depend upon things other than the relative positions. In this way arise such concepts as viscous forces. So wedded are we to the language which we have created, and in terms of which we think, that we will go to almost any length to preserve it. Thus, in the case of electrodynamics, the principles of the subject have led to a form of statement of the law of motion of an electron in which we say that the electron moves in such a way that

$$\iiint \varrho\left(\mathbf{E}+\frac{\mathbf{u}\times\mathbf{H}}{c}\right)d\tau=0$$

where $\mathbf{E}$ and $\mathbf{H}$ are the electric and magnetic fields, ϱ is the electric density, $\mathbf{u}$ the velocity of an element of the electron, and the integral is extended all over the electron.

We do not like this statement. It does not look like an equation of motion, so we first divide the nothing into two parts (a large portion of theoretical physics is derived by dividing nothing up into two parts) and write it as

$$\iiint \varrho\left(\mathbf{E}_o+\frac{\mathbf{u}\times\mathbf{H}_o}{c}\right)d\tau+\iiint \varrho\left(\mathbf{E}_i+\frac{\mathbf{u}\times\mathbf{H}_i}{c}\right)d\tau=0$$

where subscript i denotes the field of the electron's charge, and subscript o the field of the other charges of the system. The integral involving the subscript i is of course calculable when we know the shape and change of shape of the electron with motion; and when we work it out and expand it as a function of the velocity, acceleration and higher time derivatives, we obtain, even for the case where motion is rectilinear, an equation of the form

$$\iiint \varrho\left(\mathbf{E}_o+\frac{\mathbf{u}\times\mathbf{H}_o}{c}\right)d\tau=a_1\ddot{\mathbf{x}}-a_2\dddot{\mathbf{x}}+\cdots \text{etc.}$$

where the a's are functions of the velocity of the electron. We thus have an equation in which the acceleration of the electron appears. All the rest of the terms are transferred to the left-hand side and christened "Force," the term

$$\iiint \varrho\left(\mathbf{E}_o+\frac{\mathbf{u}\times\mathbf{H}_o}{c}\right)d\tau$$

being the external force, and the other terms radiation reactions. Thus is our language "Force equals mass times acceleration" maintained. It is true that since the coefficient of the acceleration is a function of the velocity it is necessary to speak of a mass varying with the velocity; and, moreover, since that coefficient of the acceleration is different when the velocity is perpendicular to the acceleration from what it is when it is parallel thereto, it is necessary to speak of a longitudinal and a transverse mass.

The limit of straining of ideas would of course be reached if the equation of motion had not involved the acceleration at all. We could still have inserted it on the right-hand side provided that we had put in a corresponding term on the left-hand side to cancel it, but nobody would believe that the formal retention of the words "Force equals mass acceleration" for this case would provide any simplification.

I of course grant that the smallness of the variation of mass with velocity, and the relative unimportance of the terms other than the acceleration term in the usual cases where the equation is employed afford a justification for the process in the ordinary applications to electron theory, and I simply discuss the case because the complications involved in retaining formal adherence to the ideas of force and mass acceleration give, even in this case, a fairly strong threat of the trouble we invite, if we expect to be able to throw every equation of motion which we encounter into this form.

To many minds the appeal of some sort of minimum principle is almost as strong as the more primitive appeal of forces, kinetic reactions and the like. The earliest of these principles is the principle of least time, so well known in optics, a principle which is, of course, the analytical equivalent of the ordinary laws of reflection and refraction. Probably the primary cause which has given rise to the desire for minimum principles has been a feeling that a properly designed universe would naturally be constructed so that something should be as small as possible. It should not be wasted; and the problem remains to discover what it is that is of such value in the universe that it should not be wasted in this sense. The dignity of the process suffers to some extent when we recall that, since the condition that the time integral of a certain function of certain variables shall be a minimum is a set of differential equations, it is not surprising that given any set of differential equations describing the motions of bodies we should be able to construct an integral for which these equations are the conditions that it shall be a minimum. After the discovery of such an integral it only remains to christen it, and its importance in the science becomes established. The minimum principles expressed in the Hamiltonian principle and the principle of least action are of course merely the analytical equivalents of the equations of motion of Lagrange; and, in spite of the fact that almost every expounder of a new theory tries to put his equations in the form of a minimum principle, it is doubtful whether any greater

significance is to be attached to the process than a crystallization of the equations in a compact form.

A characteristic feature of theoretical physics has been the moulding of new theories in the forms of those which have gone before, by a judicious christening of the quantities concerned. One of the classical examples of the process is to be found in Maxwell's moulding of the laws of electrodynamics into the form of the equations of ordinary dynamics. To some, this achievement appears as a proof of the electrodynamic laws from dynamical principles; but a careful perusal of what has been done will show rather that it constitutes a discovery of what quantities it is necessary to christen kinetic and potential energies, and in terms of what coordinates it is necessary to talk in order that the application of the dynamical principles in the form of the equations of Lagrange, or, in later developments, in the form of the Hamiltonian principle, shall lead to the results stated in the laws of electrodynamics.

Definition and Scaffolding in Physical Theory

The content of a physical theory is frequently obscured owing to the fact that certain of the quantities occurring in it are visualized in one sense, defined in another and used in yet another. To illustrate my point, let me for a moment remind you of the well-known equations of electrodynamics

$$\frac{1}{c}\left(\varrho\,\mathbf{u} + \frac{\partial \mathbf{E}}{\partial t}\right) = \operatorname{curl}\mathbf{H} \quad (1); \qquad -\frac{1}{c}\frac{\partial \mathbf{H}}{\partial t} = \operatorname{curl}\mathbf{E} \quad (3)$$

$$\varrho = \operatorname{div.}\mathbf{E} \quad (2); \qquad 0 = \operatorname{div.}\mathbf{H} \quad (4)$$

$$\iiint \varrho\left(\mathbf{E} + \frac{\mathbf{u}\times\mathbf{H}}{c}\right) d\tau = 0 \quad (5)$$

where the triple integral is extended over the whole of one electron. Now to one who has pictured the moulding of these equations from dynamical principles, it will be recalled that **E** is visualized as an ethereal displacement, while equations (1) and (4) really constitute the definition of **H**. On the basis of another form of dynamical development applicable to the case where no charges are present, **H** appears as a velocity of the ether, while **E** appears as the curl of an ethereal displacement. On the other hand **H** is frequently defined as the force on a unit pole and **E** the force on a unit charge. On the basis of a separate definition of all the quantities concerned, each of these equations may be regarded as the expression of a law which might or might not be true. But how is **H**, for example, *used,* in most of the important applications of these equations at any rate? Certainly not as the force on a magnetic pole. For the realization of a magnetic pole involves the conception of a magnet infinitely thin in the macroscopic sense, but thick enough in the sub-macroscopic sense to contain a large number of molecules per unit of length, for example. Now when we are talking about the magnetic field at a point inside an electron, and we do have to talk of this in many problems in electrodynamics, how are we to conceive of that enormously large magnetic pole, which indeed only has existence in the macroscopic sense, as being placed in an element of volume of the electron which is infinitesimal in the sub-macroscopic sense. The point involved is not one of experimental difficulty, but of logical absurdity. Yet, if we do not trouble ourselves about the matter, the calculations go along merrily as though all were well. They do this because we really do not use the quantities **E** and **H** in the sense in which the unit charge and pole defined them. If we solve the equations (1) to (4) the solutions can be put in the well-known form

$$\mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{U}}{\partial t} - \operatorname{grad}\varphi \quad (6); \qquad \mathbf{H} = \operatorname{curl}\mathbf{U} \quad (7)$$

where

$$4\pi c\mathbf{U} = \iiint \frac{[\varrho\mathbf{u}]}{r}\,d\tau \quad (8); \qquad 4\pi\varphi = \iiint \frac{[\varrho]}{r}\,d\tau \quad (9)$$

and where the square brackets indicate retarded potentials.[3] The content of the scheme is then contained in the statement that the motion of the electron is such that

$$\iiint \varrho\left(\mathbf{E} + \frac{\mathbf{u}\times\mathbf{H}}{c}\right) d\tau = 0$$

where the integral is extended over the electron in question, with the understanding that the values of **E** and **H** are to be calculated by (6) to (9), by taking account of the contribution from all the electricity, including that in the electron whose motion is under investigation. The law really expresses how the electrons move in relation to one another. The vectors **E** and **H** constitute the scaffolding for relating the motions of the charges. In practice, as already stated, the plan is to separate the integral into two parts—one corresponding to the field of all the electrons other than the electron under investigation and another part contributed by that electron itself, this latter part being then expressed as a func-

[3] The restricting relation $\operatorname{div}\mathbf{U} = -\frac{1}{c}\frac{\partial\varphi}{\partial t}$, which is not here written down, but which holds between φ and **U**, merely provides for the equation of continuity, in preventing us from assigning in our problem change motions and density changes which might violate that equation, an equation which is contained in (1) and (2) themselves. If we automatically provide for the equation of continuity in stating our problem, the restricting relation between φ and **U** need not be stated explicitly.

tion of the motion of the electron. In this way, the motion of the electron is related to the fields of the other electrons.

To any one who would object to the use of quantities, or symbols such as $\mathbf{E}$ and $\mathbf{H}$ in respect to which no definition has been given, let me cite the following very simple example. Suppose I desire to express the equation of a parabola in parametric form. I may write

$$x = us \qquad (10)$$

$$y = \frac{1}{2} g s^2 \qquad (11)$$

and state that the desired curve, which is of course $x^2 = (2u^2/g)y^2$, is to be obtained by eliminating s. Nobody asks me what s is. If by chance they do, I reply that it is merely a parameter which becomes eliminated in the final result, and the equations which have been written down constitute a definition of a process of calculation rather than a relation between magnitudes. If I am pressed for a definition, I shall tell my inquisitor to take the first of the equations as the definition of s. He will then be happy, but will make a calculation no different from mine. His morale would probably be improved enormously if I permitted him to believe that the curve under discussion was that of a body with horizontal velocity u, falling with acceleration g, for he would then be able to regard s as the time, but that would not alter his curve.

In the same way as equations (10) and (11) define a process of calculating the relation between x and y, so equations (1) to (5) define a process of calculating the relation between the motions of the various charges of the system. If I am pressed for a definition of $\mathbf{E}$ and $\mathbf{H}$, I define them by equations (1) to (4) or, what is the same thing, by (6) and (7). If anybody asks me whether equations (1) to (4) are right, I do not know what he means. Of course they are right. They constitute the method of calculating $\mathbf{E}$ and $\mathbf{H}$. Truly, it may turn out that the quantities calculated may be of no use; and the essence of the whole scheme lies in the fact that it states that the motion of an electron may be evaluated in terms of the $\mathbf{E}$ and $\mathbf{H}$ so calculated from the motions of the other electrons. Any disagreement of the classical electrodynamics with the quantum theory or anything else is to be sought not in equations (1) to (4), which are usually regarded as most characteristic of it, but in the force equation (5).

CONSERVATION OF ENERGY IN ELECTRODYNAMICS AND IN EXPERIENCE

Now it is a pure analytical consequence of the circuital relations that for any field obeying them,

$$\iiint \varrho\left(\mathbf{u}.\,\overline{\mathbf{E} + \frac{\mathbf{u}\times\mathbf{H}}{c}}\right) d\tau = -\frac{\partial}{\partial t}\iiint \frac{1}{2}(\mathbf{E}^2+\mathbf{H}^2)\, d\tau - c\iint (\mathbf{E}\times\mathbf{H})_n\, dS \qquad (12)$$

where the volume integral is extended throughout any volume, and the surface integral over the corresponding surface. This equation suggests what quantities it is appropriate to christen energy density, energy flux and so forth in order that the form of the principle of energy may be preserved. If all parts of an electron move with the same velocity so that the vanishing of

$$\iiint \varrho\left(\mathbf{E} + \frac{\mathbf{u}\times\mathbf{H}}{c}\right) d\tau$$

insures also the vanishing of

$$\iiint \varrho\left(\mathbf{u}\cdot\overline{\mathbf{E} + \frac{\mathbf{u}\times\mathbf{H}}{c}}\right) d\tau$$

for each electron, then the addition of the force equation (5) insures that the left-hand side of (12) shall be zero, and a christening of $c(\mathbf{E}\times\mathbf{H})$ as flux of energy per square cm and $\frac{1}{2}(\mathbf{E}^2+\mathbf{H}^2)$ as energy per cc leaves us with the statement that the inward flux of energy through a surface is equal to the rate of increase of energy within it, which energy in this case works out approximately as $\Sigma\frac{1}{2}mv^2$ for all the electrons within the surface. If all parts of the electron do not move with the same velocity, the left-hand side does not vanish, and what remains of it must be associated with the other volume integral as a rate of increase of the internal energy of the electron in order that the language of the conservation of energy shall be maintained. Now directly the force equation ceases to be true, and it certainly is not true in problems dealing with photoelectric effect and the like, all this evaporates. It even evaporates partially with the force equation holding, in the practical case where the velocities of different parts of the electron are not the same. This does not mean that any astonishing violation of the principles of nature is involved, but simply that, for the cases where the force equation does not hold, the quantity $\frac{1}{2}(\mathbf{E}^2+\mathbf{H}^2)$ and the Poynting flux $c\,(\mathbf{E}+\mathbf{H})$ are unsuitable quantities to have been christened as we have christened them. When we are speaking of the conservation of energy, I think we have to realize that the vital point at issue is this. Suppose we have a self-contained system in which are to be found a number of electrons moving with constant, though possibly different velocities, and we measure $\Sigma\frac{1}{2}mv^2$ or, if we like, the more elaborate expression

$$T=\Sigma\, m_0c^2\left[\left(1-\frac{v^2}{c^2}\right)^{-\frac{1}{2}}-1\right]$$

corresponding to the theory of relativity. Then suppose that after these electrons have bombarded the various things around, and a steady state in which the velocity of each electron is once more constant has been attained, we once more measure T for all the electrons in the system. In the spirit of its practical application, all that conservation of energy requires is that the sum shall be the same as before. Now such a condition is provided for by the most radical forms of the quantum theory. When we speak of the quantum theory violating the spirit of the conservation of energy, our thoughts are apt to be concentrated upon the idea of how it may be possible for the Poynting vector of electromagnetic theory to pump as much $\Sigma\frac{1}{2}mv^2$ into some photoelectron as it appears to get. The trouble here is not with the conservation of energy, but with the picture of how we would like the process to occur. If it turns out that the things which long ago were christened energy flux and energy density do not have the properties which energy flux and energy density should have, the trouble is to be found in the christening.

The Present Status of the Circuital Relations

If, however, the force equation is wrong, and that is the only part of the electromagnetic theory which is not scaffolding, what ought to be done with the scaffolding? May it not serve for another structure? It possesses some very desirable features. It is true that these features lie in the forms of the equations rather than in any connection which they may have with electricity. For the valuable features of the equations are preserved if we should replace ϱu_x, ϱu_y, ϱu_z and ϱ by anything else, as, for example, some set of quantities S_x, S_y, S_z, S_t. All that the equations will require of this set of quantities is that it shall obey the relation

$$\frac{\partial S_x}{\partial x}+\frac{\partial S_y}{\partial y}+\frac{\partial S_z}{\partial z}+\frac{\partial S_t}{\partial t}=0$$

which, in the electrical case, is the equation of continuity. The equations will talk as glibly in terms of the quantity S assigned as a function of x, y, x, and t as, in electromagnetic theory, it speaks in terms of ϱu and ϱ.

The valuable feature of the equations is of course contained in those properties which, if we were speaking in a language which talked of transference of energy through an ether, we should term the properties necessary to produce transverse waves.

Now suppose that the equations be applied to any problem where interference patterns are obtained, such as the problem of a point source and a mirror. What certainly is true is that where the equations predict a zero value for **E** nothing ever happens, photographic, photoelectric or visual. Where they predict a value other than zero, something may happen; for example, a photoelectron may be ejected from an atom there. The unsatisfactory element in the situation lies in the fact that we should have preferred to say with certainty that the event will or will not occur. The indecision of the statement might be converted to certainty by supposing that something about the electron or the atom in which it existed determined whether it would or would not be ejected. This solution seems psychologically repugnant to the mind. For one thing it would violate the spirit of the conservation of energy, not necessarily, it is true, in a practical form in which we discuss it only statistically, but in a form which many would like to retain. For, according to it, that particular phenomenon which we speak of as the emission of a quantum from a radiating atom and caused possibly by the absorption from an electron of $\frac{1}{2}mv^2$ equal to $h\nu$ might, occasionally, be accompanied by the emission of more than one electron from other atoms, each with gain of $\frac{1}{2}mv^2$ equal to $h\nu$, although of course in the statistical case which concerned a large number of atoms, practical conservation might be obtained.

Not only does the scheme of circuital relations predict where photoelectric and other phenomena will certainly not occur and where they may, but the probability of occurrence of one of these phenomena in unit time in any element of volume as a result of the emission of a quantum of radiation from some point is, for a given frequency and type of phenomenon, proportional to the vector which on classical electromagnetic theory represents the Poynting flux of the primary wave there, although the actual $\frac{1}{2}mv^2$ gained by a single electron may be equal to the time integral over the period of emission of the surface integral of the Poynting flux over the whole surface of a sphere drawn with the source as origin.

The crude notion of a quantum of energy shot out from an atom in the form of a ball or of a wave filament provides a satisfying picture as regards an interpretation of the inner significance of the probability, which otherwise stands as a sort of confession of ignorance, and it provides a picture by which the mind can think of the energy as being handed on from point to point from source to goal. It does this, however, at the expense of enormous complica-

tions as regards interference. Moreover, it has other difficulties. In its primitive form, the small ball notion is inconsistent with interference experiments altogether; and as regards the filamentary wave, we have the difficulty pointed out by Lorentz that Michelson's experiments on interference over long paths show that the quantum must have a considerable length, and his measurements of the diameter of stars, in which interference is obtained between two portions of a wave front a considerable distance apart show that the quantum must have by no means a small width, so that altogether it is a pretty big thing. Then, the experiments of Wien and Dempster on the decay of the light emitted by positive rays during their journey along a vacuum tube suggest that appreciable emission occurs over a period of the order of 10^{-8} second. Yet nobody doubts that if he should shut off half of the path of emission, and use the light of the tail end of the pulse (if it is a pulse) to produce a photoelectric effect, he would get the full velocity of electronic emission predicted by the Einstein formula.

How then do we stand in this matter? One thing is certain. If we believe that the equations of classical electrodynamics mark out the space in such a way that where they predict darkness, nothing ever happens, any quanta emitted from the source must follow paths such that they never cross a region where darkness is predicted by the classical theory. In other words, the quanta must follow the paths of that vector which in classical theory was called the Poynting flux, and which even with the abandonment of its meaning as a flux of energy still has a definable direction and magnitude. A full appreciation of the artificiality of the belief that all things when left to themselves should travel in straight lines with constant velocities will leave one who is sufficiently sophisticated with no difficulty as to the logical possibility of the quanta traveling along paths which are not straight. To make a long story short it would appear that the following picture would provide a satisfactory correlation of most of the phenomena in terms which should be agreeable to one who wishes to retain the idea of a special handing on of energy from source to goal.

A Possible Correlation of Classical Theory with Quantum Emission

We recall that the quantum theory speaks in terms of the existence of certain stationary states of electronic motion in the atom, in which no energy is emitted, the emission of energy occurring only during the transition from one state to another. Now even in the stationary states there is no question of the existence of a field at external points in space. This is there as a matter of definition even though the field should not do anything. We are under no compulsion to imagine that the field is associated with the radiation of energy in the sense that the electron should have to alter its orbit. I am not particularly concerned with the question of whether this field is allowed to do anything or not. Perhaps there is something to be said on the side of consistency for allowing it to do something, so that the atomic orbits may influence each other, and provide, for example, for such a phenomenon as the magnetization of one substance by the presence of another, an effect ultimately attributable to interactions between the orbits in the stationary states. Any actions of this kind which we imagine to take place will have to be founded on equations of motion which give rise to no trouble. Thus, for example, the equation of motion

$$e\left(\mathbf{E}+\frac{\mathbf{u}\times\mathbf{H}}{c}\right)=\frac{d}{dt}\frac{m_0\mathbf{u}}{\left[1-\frac{u_x^2+u_y^2+u_z^2}{c^2}\right]^{1/2}}$$

for an electron, where $\mathbf{E}$ and $\mathbf{H}$ refer to the external field, is an equation consistent with restricted relativity and one which would permit the existence of the permanent orbital motion of an electron about a nucleus without diminution of amplitude, since the terms involving higher time derivatives which occur in the complete equations of classical dynamics but which have never been observed experimentally are absent. Interactions between different orbits may take place and will give rise to no trouble of a fundamental nature, except perhaps in cases of complete resonance. The acquirement of energy by free electrons as a result of the fields due to the orbital motions presents some difficulties in the matter of realization of a steady state. However, I do not intend to concern myself greatly with these difficulties. If they become too acute, the avenue of escape lies through denial of any influence between the electrons during their steady state motion, and the main thing which I desire to imply is that the course of events in the picture we have sketched is one of which we would have but little direct experimental knowledge. Hardly any of the phenomena with which our experiments deal would be realized. No electrons would be ejected from an atom so as to give rise to photoelectric effect, no photographic action would take place, and the eye, which operates, presumably, through photoelectric agency, would be unconscious of what was happening.

Now suppose that, owing to some agency, for example, the advent of a high speed electron from somewhere an electron transition from one orbit to an-

other occurs in an atom, and suppose that somehow or other unifrequentic radiation is emitted during this process of transition. I should perhaps not use the words radiation of energy, for all I mean is that the field changes produced outside the atom are those corresponding to an electron moving with simple harmonic motion. We shall suppose that this field is calculable from strict classical theory; but, in calculating its effect on other atoms, we shall suppose the electrons there replaced by virtual linear oscillators in the sense prescribed by Bohr, Kramers and Slater.[4] The use of these virtual oscillators must, of course, be regarded as a temporary expedient designed to enable us to proceed in our speculations without awaiting a complete solution of the story of how the atoms influence each other. Through the agency of these virtual oscillators, as Bohr, Kramers and Slater show, all the phenomena of classical theory, such as reflection, refraction and polarization, take place. The virtual oscillators of the reflecting mirror produce waves which conspire with the waves originating in the primary transition in such a way as to produce reflection. If there be polarizing media present, the electronic motions set up in these are such as to cooperate with the primary field, again in true classical manner. The peculiar thing about the whole situation is that we should be all unconscious of it. For none of these phenomena are associated with things we observe. It is not necessary for us to regard these fields as carrying energy and therefore invite trouble in the matter of accounting for it. But now suppose that during the transition something else happens. Let us suppose that a quantum, in the form of a ball if we like, is emitted from the electron, and let us suppose that its probability of emission in a fixed element of time, at any instant, and from any element of surface, is proportional to the magnitude, at that time and place, of the vector, which in classical theory would be the Poynting vector associated with the *irreversible radiation field* of the virtual oscillator,[5] the magnitude of the quantum being, of course, $h\nu$. The factor of proportionality in the expression for the probability is of course such that the probability that quantum is emitted somewhere at some time during the transition is unity. Let us suppose that the quantum then follows the path of the Poynting vector[6] emanating from its point of emission, this Poynting vector being constructed from the resultant field composed of the radiation field associated with the initial transition and all the secondary fields to which it gives rise through its action on the virtual oscillators in the other atoms which are influenced by it. If, following the line of a particular Poynting vector, the quantum comes into the vicinity of one of the electrons of a virtual oscillator, we expect that it may be caught by the electron in such a way as to enable it to induce a transition there if its energy is greater than that necessary for the transition. The effects of this transition, at least in the limiting case of ionization, are the things physically observable as photographic, photoelectric or visual effects.

As regards their practical aspects, all interference phenomana follow as in the classical theory, except that there is no interference of quanta. At the places where the classical theory indicates darkness there is darkness on the present view because no quanta succeed in getting there. Polarization phenomena do not invoke any polarization in the quanta themselves, but are provided for by the polarization of the vectors **E** and **H** as calculated by the classical process. All that is concerned in the question of whether or not quanta shall enter any region of space is the question of whether or not there is any path leading there from the place of the original transition by way of a finite Poynting vector.

An experiment such as that of Wien on the decay

[4] The quantum theory of radiation, *Phil. Mag.*, S-6, Vol. 47, pp. 785–802, 1924.

[5] It may be a comfort to some to regard the field originating in the transition and operating on the virtual oscillators as consisting of vectors of a kind different from those to which the electrons give rise during their motions in their stationary states, although we may suppose them to obey the same laws in relation to their determination in terms of the singularities which produce them. One may even go so far as to postulate, as operating to produce the field associated with the transition, a singularity other than the electron, which singularity moreover performs the function of the moving element in the virtual oscillator. Its binding to the ordinary electron which makes the transition exists simply in its acting as a sort of trustee for the quantum which it receives from the electron and disposes of according to its own judgment as specified in the law of probability here stated. It may be that we shall have to look to the nucleus for the seat of the virtual oscillator.

[6] Since this was written, there has come to my attention a paper by Louis de Broglie, *Phil. Mag.*, S. 6, Vol. 47, pp. 446–458, 1924. In this paper the author presents the idea of light quanta following the paths of the "rays" of optics, an idea related in some degree to the above. The details of the theory are, however, quite different from that here sketched. I must also mention that during the meeting at which the above address was presented, I learned in conversation with Dr. Slater that, in work as yet unpublished, he had given some attention to the matter of light quanta following the Poynting vector.

of intensity along the paths of the positive rays receives a natural explanation from the fact that during the emission of the unifrequentic radiation the amplitude of the vibrations and the magnitude of the Poynting vector diminishes, so that in regions of the beam far from the point of excitation the observed intensity is small because the Poynting vector is small there, and the probability of emission of the quantum at those places is small compared with its probability of emission at an earlier stage. The quantum, for a given frequency, is the same thing wherever it is emitted, however, so that its property of ejecting electrons with the true Einstein frequency does not depend upon the point of emission along the beam, and of course only one quantum is emitted for each transition. It is of interest to observe that the Wien experiment points to a real damping of the unifrequentic oscillation of the virtual oscillator.

The picture here presented permits us to indulge in a rather closer speculation as to the mechanism of the processes involved. For, if, in free space, we travel with the energy along a tube whose walls are composed of the Poynting vector, and measure the area of cross-section of the tube as we go along, the product of the area and the Poynting vector will be constant, so that the probability that a quantum emitted with and following the Poynting flux shall pass within any given area perpendicular to the flux, in any given time, is proportional to the quantity which on classical electromagnetic theory would represent the energy which has passed through that area in that time. Now, what happens when absorbing atoms are present in the field? Suppose we imagine such an atom replaced by a virtual oscillator and consider the vibrating electron acting as one constituent of the oscillator. The motion of this electron under the influence of the field will of course bring about such a resultant orientation of the Poynting vector about itself as to cause "energy"[7] (in the classical sense) to feed into its vicinity.[8] The greater the absorption of energy in the classical sense, the greater the probability that the quantum emanating from the original center of disturbance shall be directed towards the absorbing atom in such a way as to produce a transition. In the case of resonance of the virtual oscillator, the probability that the particular atom would receive the quantum would be enhanced, since resonance enhances the flux of the Poynting vector into the region occupied by the oscillator.

A complete theory following the lines above indicated would involve specification of the conditions which determine the capture of a quantum by the atom. One is immediately tempted to set up a correlation between the absorption of energy by the oscillator in the classical sense (the fizzling out of the Poynting vector in the regions around the oscillator), and the capture of the quantum. Detailed discussion of this matter is impossible without going into great length. However, it would appear that reasonable assumptions, designed to maintain the principle that the probability of the passage of a quantum through a given element of area shall be represented by the ratio of the time integral of the total Poynting flux across that area to the time integral of the total Poynting flux from the original center of disturbance, would also provide for the conclusion that the probability that a quantum of suitable energy would produce a transition in any given atom is equal to the ratio of the "classical energy" which that atom would absorb from the wave train emitted from the center of disturbance to the total "classical energy" of that wave train. Such a law would appear to be in harmony with experience.

One may demur against our refusal to regard the Poynting vector itself as a true flux of energy, on the basis that the parts of the virtual oscillator are actually set in motion in such a way as to relate their energy (in the $\frac{1}{2}mv^2$ sense) to what on classical theory would be the energy flux represented by the Poynting vector. The objection is to be met by the statement that this motion disappears as the oscillation of the virtual oscillator dies down. It never becomes apparent to us, and does not figure in a scheme which regards the only experimentally recognizable phenomena as those resulting from ejection of electrons from their orbits, or more generally the acquirement of energy by electrons in quanta.

An interesting situation arises when the quantum, following the path of the Poynting vector, gets caught in the vicinity of an atom and finds itself without enough energy to produce a transition, or when it gets caught in the vicinity of a free electron. We must suppose that, in the former case, it becomes scattered, unaltered, while in the latter case it gives rise to the Compton effect. A characteristic feature of the Compton effect is the emission of different

[7] We use the term "classical energy" for want of better words to denote the integral of $1/2(E^2+H^2)$ over the region concerned, although, as already implied, we are not regarding this as in any way the equivalent of the energy transferred by the quantum. In fact we are not regarding it as energy at all.

[8] Since this address was delivered my attention has been called to a paper by Richard Becker, *ZS. f. Phys.*, 27, 3, p. 173, 1924, in which the probability of a transition is made to depend upon the surface integral of the Poynting flux taken all over a sphere surrounding the atom. The author does not appear to use the idea in connection with quanta following the Poynting vector, however.

frequencies in different directions. The situation in this connection has been discussed by Bohr, Kramers and Slater,[9] who, in representing the action of the free electron, invoke a moving linear oscillator whose Doppler effect gives rise to the different frequencies, in different directions, and who, in representing scattering by an atom, invoke (presumably) a linear oscillator emitting a frequency corresponding to the primary quantum and modified now to a negligible extent by the Doppler effect. Leaving the justification of these assumptions to the arguments presented for them in the paper referred to, we may incorporate the result in a theory of quanta of the type here sketched, for the purpose of correlating the phenomena of the Compton effect and atomic scattering, by supposing that the energy of the original quantum, having been received by the system, becomes available for total or partial reemission in varied amounts, the probability of emission of a quantum from the virtual oscillator at any point of the surface of its oscillating electron in unit time being proportional to the magnitude of the Poynting vector corresponding to the irreversible radiation emitted from that point, and the magnitude of the quantum emitted being determined by the frequency of the wave emission from that point, the subsequent history of the quantum after emission being determined in its relation to gratings, prisms, etc., upon which it may fall by the same sort of considerations as those which determine the history of the primary quantum.

It is then necessary to endow the quanta with the characteristics of momentum in such a way that the energy and direction of emission of the electron associated with the scattering follows as in Compton's calculations. The feature which a view of this kind adds to the more primitive theory is first, in harmony with the view of Bohr, Kramers and Slater, its consistency with the undulatory phenomena required by the properties of the scattered radiation in relation to its analysis by a grating, and second, its formal attempt to assign definite probabilities to the scattering of quanta of different magnitudes, and in different directions, these probabilities being calculable in terms of the magnitudes of the Poynting vector at the appropriate point of emission in the sense outlined above.

Apart from its power to provide a visual picture of the passage of energy from one place to another, its power to give through the law determined by the Poynting vector a physical interpretation of the probability of a transition induced by the quantum, and a numerical magnitude to that probability, the concept of a quantum operating according to the laws above described provides the feature of an instantaneous emission of the energy, in spite of the association of that emission with a wave emission extending over a finite time. Such a feature as the last named seems essential to a satisfactory explanation of the fact that, in such a phenomenon as that of the Wien experiment above cited, the "light" emanating from any part of the track of the positive ray, no matter how small, possesses the power to produce the full Einstein photoelectric velocity in any electron which it ejects from an atom in its path.

Finally, it may again be emphasized that if we decide to talk in terms of quanta at all, and accept as an experimental fact that no quantum ever crosses a region where the classical theory predicts darkness, in other words, if we accept the classical theory as an empirical description of interference phenomena and the like, we practically constrain the quanta to follow the paths of the Poynting vector in the sense outlined above.

W. F. G. Swann

Yale University

(To be concluded)

[9] *Loc. cit.*

THE BIOLOGICAL SURVEY OF THE MOUNT DESERT ISLAND (MAINE) REGION[1]

This survey was undertaken in the season of 1923 at the suggestion of Mr. William Procter, a member of the board of trustees of the laboratory. The object of the survey is to gain a knowledge of the flora and fauna of the region, principally the marine forms, which will be of use to the scientific research workers who contemplate coming to work at the laboratory, as well as to present a picture of the ecology of the forms, the numbers as to kinds and individuals, their distribution with regard to season of year and over periods of years, kinds of water and bottoms that they live in, temperature conditions that influence them, their feeding habits, mating habits and seasons, habits of offense and defense and other ecological relationships.

The work may be divided into two more or less distinct parts: First, a series of intensive surveys by individual workers of restricted areas, and second, a more general and comprehensive treatment of the whole area which will take a much longer period of time but from which interesting results have already become apparent.

In the first part the following papers have already been published or are in course of preparation:

[1] By the members of the staff of the Mount Desert Island Biological Laboratory and Associated Naturalists.

The zonation of vegetation on a rocky coast: Duncan S. Johnson and Alexander F. Skutch.

Symbiosis between a green algae and an amphipod: Alexander F. Skutch.

A botanical survey of North-East Creek, Mt. Desert Island: Paul Acquarone.

The zoological grouping of forms on the sea-bottom on Cod-Ledge off Mount Desert Island: Roy Miner. To be reconstructed in models as a group in the American Museum of Natural History, N. Y.

The rotifers of Mount Desert Island: Frank J. Myers and H. K. Harring. To be published in the Trans. Wis. Acad. of Science.

The primitive luminous organisms of Maine: Ulric Dahlgren. *The Maine Naturalist,* Vol. IV, Nr. 1, 1924.

The fireflies of Maine: Ulric Dahlgren. *The Maine Naturalist,* Vol. II, Nos. 3 and 4, 1922.

The larval distributive habits of some Pelecypod mollusks of the waters surrounding Mount Desert Island: Thurlow Nelson.

The insect fauna of Mount Desert Island: Chas. W. Johnson.

In the general work some 50 collecting trips have resulted in the recording of about 250 forms of marine animals on form cards, with notes as to their location, abundance, breeding, food, enemies, parasites, migrations, etc. This work has been pursued mostly in the neighborhoods nearest the laboratory and a lesser part in the deeper water south of Otter Creek and around Egg-Rock. Some of the more interesting facts that have emerged, for further study as to causes, are as follows:

Yearly variation in the abundance of *Aurelia aurita.* This jellyfish was very abundant in Frenchman's Bay in 1921. In 1922 it disappeared to such an extent that but two specimens were seen at Southwest Harbor. In 1923 some few specimens were noted and recorded in the bay. In 1924 it appeared in moderate abundance. It is evidently coming back to its normal abundance and it seems very probable that in 1925 it will be found in large numbers again. The cause of this variation is unknown, but a possible solution of the question is that the heavy ice may mechanically kill the attached hydroid stages or hyphae. Year by year this question will be studied in search of an explanation.

In the same way in 1921 and 1922 the tubularian hydroids, especially *Tubularia crocea,* were very abundant, covering the spiles of docks with huge masses. In 1922 the nudibranch mollusk *Dendronotus frondosa,* which was only moderately abundant in 1921 and which lives principally on tubularian hydroids, had become extremely numerous and was found all over the hydroids and crawling everywhere. By 1923 the hydroids were so scarce that a few specimens were found with difficulty and in that summer *Dendronotus* had also declined in numbers. In 1924 but six small specimens of *Dendronotus* were found. The relationship here was that of food supply and suppression of the hydroids by overeating by *Dendronotus,* followed by suppression of *Dendronotus* by starvation in the absence of the hydroids.

The question of yearly temperature relationship was well exemplified by the localization of the pelagic shrimp *Nyctiphanes norvegica.* In 1921–1922 this shrimp was rarely seen in the bay, although known to be a common inhabitant of the cold outside waters, where it lives in great schools near the bottom.

The winter of 1922–1923 was very cold and the spring very late so that in June, 1923, the waters of Frenchman's Bay were far below the normal in temperature. This resulted in the invasion of the bay by myriads of *Nyctiphanes* which swarmed on the surface at night and swam in the mid-depths of the lower bay around Bald-Rock in the daytime. This was repeated in the summer of 1924 after a cold late spring.

An interesting and undescribed breeding habit of a marine annelid worm has been observed and recorded for several years and further study is necessary. This form is a tube builder living in moderately deep water near the entrance of the bay. It breeds several times each summer, these periods being about a month apart and thus showing the periodicity that is so characteristic of the *Palolo* worm, *Odontosyllis, Nereis* and many other marine annelids. On the day when it breeds the red eggs are cast in untold numbers into the bottom waters and during a rapid development rise to the surface at about the middle of the day. The water becomes blood red and as the day lengthens, toward evening, the active little trochophore larvae suddenly become negatively heliotropic and move toward the bottom, leaving the water of natural color again.

The breeding, seasonal succession and migrations of several of our important food fishes have been observed but not as yet seriously studied. This important feature must wait until the laboratory can secure a better boat than it now owns.

Numbers of interesting animal associations have been recorded and lists made, and the work increases in value year by year. The papers written are published in various journals until the laboratory is able to publish its own work.

Ulric Dahlgren

Princeton University

CHARLES ANTHONY GOESSMANN[1]

Charles Anthony Goessmann was born in the village of Naumburg, Electorate of Hesse-Cassel—not then absorbed by Prussia—on the thirteenth of June, 1827, and died at Amherst on the first of September, 1910, in his eighty-fourth year. Fourteen years have passed since then; yet his memory is still green in the hearts of his pupils. Admired and respected, he was also beloved. At Göttingen, Syracuse, Troy and Amherst he was always the "Beloved Goessmann." No teacher could be dearer to his students than he. Preoccupied as he was in meeting the demands of his own department and private work, he was ever ready to help others. Now and again his colleagues turned to him for counsel. And so we find Clark, Peabody, Stockbridge, Maynard and even Totten, in his important researches on compensating powder and other high explosive compounds, seeking his aid and advice in their own investigations.

He was a teacher in a wide sense. He not only taught his pupils in the class-room and laboratory and trained his assistants, but he made the college the nursery of agricultural chemists for other institutions throughout the land. By his lectures and talks, his reports and bulletins, he taught and educated the public. In the lecture-room and laboratory he was painstaking and inspired his students to grasp the problems he set before them. As an experimenter he had readiness and skill and could attain important results with the minimum possible means. No one who came in contact with him could fail to be struck with the accuracy and extent of his knowledge and the clearness of his intellectual vision.

At Göttingen he devoted himself to the discovery of new truths. After he came to America the *utility of science*, especially in his chosen field, was always uppermost in his mind. He was always tracing abstract principles to their practical applications and thus bringing scientific knowledge within reach of the farmer and the general public. Quick to read the signs of the times, he had a clear comprehension of the actual conditions and the needs of chemical education in this country.

Admirably fitted by tradition, training, experience and temperament for the life of a teacher and investigator, he brought to the service of the college a singularly happy combination of qualities—genuine devotion to his subject, great capacity for work, the power to kindle enthusiasm in others, a well-balanced mind and body and a robust physique. In the retrospect of his life one is struck with the amount of labor which he performed. Always at work, never in haste, systematic beyond most men, perfect order pervaded all that he did. His researches embrace a wide range in chemical science, and in analytical, technical and agricultural chemistry are marked by high attainment. He was not a writer of books, yet his pen was seldom idle. His first contribution to chemical science appeared in 1853, and thereafter an uninterrupted series of contributions to chemistry flowed from his pen for fifty-four years. They remain an enduring monument to their author.

Deeply religious from his youth, his was the spirit of a reverent seeker after truth, and his life was devoted to its exposition. He was a fine example of the Christian philosopher.

Frederick Tuckerman
Amherst, Massachusetts

[1] From an address delivered at the opening of the Goessmann Laboratory, Massachusetts Agricultural College, Amherst, October 3, 1924.

SCIENTIFIC EVENTS

THE INTERNATIONAL GEOGRAPHICAL CONGRESS

The International Geographical Congress met in Cairo from April 1 to April 12, under the auspices of the International Geographical Union, formed in Brussels in July, 1922.

An advanced notice published in the London *Times* states that before the war international congresses of this nature were held in different countries at more or less regular intervals, the last being the tenth congress, held at Rome. But, by decision of the International Research Council, which was formed shortly after the end of the war, the constitution of the old congress was abrogated, and the duty of organizing all future congresses was vested in the International Geographical Union in affiliation with the council.

The congress in Cairo is the first of the post-war series, and it coincides with the celebration of the jubilee of the foundation in 1875 of the Royal Geographical Society of Egypt by the Khedive Ismail. At that date Africa was still, in large part, a dark continent, though it was in 1875 that H. M. Stanley began the great journey during which he discovered the course of the River Congo. Ismail Pasha's idea was that as Cairo was the converging point both on their outward and their homeward route of various explorers, who were, with marked success, engaged in piercing the veil which for so many centuries had enshrouded the heart of this vast continent, so in Cairo there ought to be an organization which could lend material assistance and encouragement to this work and be a channel of information to the outside world. Accordingly, he granted a decree on May 19, 1875, for the creation of a society the essential objective of which was to facilitate the collection of knowledge and the exploration of the African coun-

tries, and particularly of the portions so far unexplored.

The society was financed by Ismail Pasha, who gave it a very fine library, which still exists. Its first president was the celebrated traveler Dr. Schweinfurth, whose advanced age (he is in his eighty-ninth year) prevents him from attending the jubilee of this organization, which in its early years he so helped to develop; while among those who have given before the society the earliest accounts of their discoveries, and whose reports are treasured in its archives, have been Stanley, Schweinfurth, Burton, Nordenskjöld, Junker, Gessi, Mason, Stone and Chaillé-Long.

King Fuad has always displayed the liveliest interest in the welfare of the institution founded by his father, and it is due to his initiative and to his desire fittingly to celebrate the jubilee of the Egyptian Society that the International Geographical Union agreed to select Cairo as the seat for this year's congress. The congress is under the patronage of King Fuad, who will officially open it. Its organization has been in the hands of the Royal Geographical Society of Egypt, whose president is M. Georges Foucart, Director of the "Institut Français d'Archéologie Orientale."

THE INTERNATIONAL CONGRESS OF PHOTOGRAPHY

After a lapse of fifteen years an International Congress of Photography is to be held this year from June 29 to July 4 in Paris. The last congress was held in Brussels in 1910, where a very successful meeting was held attended largely by representatives from all nations.

The congress will be divided into four sections: (1) Scientific; (2) Technical and artistic; (3) Historical and documentary; (4) Technique of cinematography (in connection with the Congress of Cinematography). A historical exhibition of photography and a centenary celebration of the beginning of photography will be held during the congress.

At the request of the International Committee of the Congress, an Organizing Committee in the United States has been formed, the members being: Mr. F. F. Renwick, Dr. W. D. Bancroft, Mr. W. H. Manahan, Mr. E. J. Wall, Dr. H. E. Ives, Professor R. W. Wood and Dr. C. E. K. Mees, chairman. A list of American patrons has also been drawn up.

The congress is especially anxious to obtain papers relating to the branches of photography with which it deals from workers in the United States. Offers of such papers can be communicated to me, and I will forward them to the secretary of the congress or they can be sent direct to M. G. Labussiere, 5 rue Brown-Sequard, Paris, XV. The secretary is anxious to know at once what contributions will be available, though it is not necessary that the whole paper should be sent to him. The title and a brief abstract should, however, be forwarded at once.

C. E. K. Mees

Eastman Kodak Company,
Rochester, N. Y.

THE COUNCIL MEETING OF THE AMERICAN CHEMICAL SOCIETY

A meeting of the council opened the sixty-ninth convention of the American Chemical Society at Baltimore, on Monday afternoon, April 6. One hundred and fourteen councilors and substitutes were present when President James F. Norris called the meeting to order and expressed his appreciation of the honor which had been conferred upon him since the last meeting.

It was voted to hold the spring meetings of 1926 and 1927, respectively, in Tulsa, Okla., and Richmond, Va. Philadelphia having been selected as the meeting place for the fiftieth anniversary convention in the autumn of 1926, it was suggested by Secretary Parsons that advantage might be taken of the sesquicentennial exposition in that city to properly portray the great advances in chemistry during the past half century in an exposition building devoted exclusively to chemistry. In this connection the court of chemical achievement to be organized in connection with the Chemical Exposition in New York this fall was cited as a good nucleus upon which to expand. The details of this proposed court of achievement were outlined briefly by H. E. Howe, the originator of the idea. Dr. McKee suggested that suitable honors be conferred upon the founder and charter members of the society living at the time of the fiftieth anniversary and that they be the guests of the society upon that occasion. Following the general discussion of plans for the fiftieth anniversary meeting, the council authorized the president of the society to appoint such special committees as may be necessary to prepare for the celebration.

Conviction that the society must not fail in the publication of a second Decennial Index to *Chemical Abstracts* characterized the discussion of this subject which was presented to the council by Secretary Parsons and Editor Crane. The problem is to finance a publication which is estimated to require six volumes for completion and will cost in the neighborhood of $90,000.

Reports from the various committees of the society were received and will be printed later.

Among the other matters of business disposed of were the granting of divisional organization to the Section of Gas and Fuel Chemistry and the acceptance

of the by-laws proposed for the new division; granting of a charter for the formation of a North Jersey Section of the society; reference of the proposed amendment to article 8 of the constitution of the society, making it possible to omit a general meeting when deemed advisable, to the membership; authorizing publication of the proceedings of local sections of the society in the *News Edition* in place of the *Journal of the American Chemical Society,* where they are now published; authorizing the elimination of addresses of new members of the society when their names are published and referring to their location according to the local section with which they are affiliated.

THE WASHINGTON MEETING OF THE AMERICAN PHYSICAL SOCIETY

THE one hundred and thirty-third regular meeting of the American Physical Society will be held in Washington, D. C., at the Bureau of Standards on Friday and Saturday, April 24 and 25. The first session will be at 10 o'clock on Friday morning. This session and the Saturday afternoon session will be held in the lecture room of the East Building. On Friday afternoon and Saturday morning there will be sessions in both the lecture room of the East Building and the lecture room of the Industrial Building.

Titles and abstracts of the papers to be presented are published in advance in the Bulletin of the society. These abstracts have not been corrected by the authors. After correction, they will be published in an early number of the *Physical Review.* There are 71 papers on the program of the Washington meeting.

On Friday evening there will be a dinner for members of the society and their friends at the Raleigh Hotel at 6:30 P. M. The price per person will be $2.50.

The one hundred and thirty-fourth regular meeting of the society will be held in Portland, Oregon, on June 19, 1925, in affiliation with the Pacific division of the American Association for the Advancement of Science. Members intending to present papers at this meeting should have titles and abstracts ready for publication, in the hands of the local secretary for the Pacific Coast, D. L. Webster, Stanford University, California, not later than Saturday, May 16.

Other meetings for the current seasons are as follows:

135. November 27–28, 1925. Chicago.

136. December 28–31, 1925, Kansas City. Annual meeting.

OFFICERS OF THE SOCIETY FOR 1925

President: Dayton C. Miller, Case School of Applied Science.

Vice-president: K. T. Compton, Princeton University.

Secretary: H. W. Webb, Columbia University.

Treasurer: G. B. Pegram, Columbia University.

Editor: G. S. Fulcher, Corning, New York

Pacific Coast Secretary: D. L. Webster, Stanford University.

Council: The president, vice-president, secretary, treasurer and managing editor; *Past Presidents:* A. A. Michelson, Carl Barus, E. L. Nichols, Henry Crew, W. F. Magie, Ernest Merritt, R. A. Millikan, J. S. Ames, Theodore Lyman, C. E. Mendenhall; *Elected Members:* H. G. Gale, D. L. Webster, F. C. Brown, A. L. Hughes, A. S. Eve, A. W. Hull, F. C. Blake and W. F. G. Swann.

THE WISTAR INSTITUTE OF ANATOMY AND BIOLOGY

THE twentieth anniversary of the organization of the advisory board of the Wistar Institute of Anatomy and Biology was celebrated at the buildings of the institute in Philadelphia on April 13. At a morning session Mr. Effingham B. Morris, president of the institute, presided and the following program was presented:

The Wistar Institute and its advisory board: DR. M. J. GREENMAN, director of The Wistar Institute.

Research at The Wistar Institute: DR. H. H. DONALDSON, professor of neurology at The Wistar Institute.

The publications of The Wistar Institute: DR. G. CARL HUBER, professor of anatomy and director of the Anatomical Institute at the University of Michigan, and DR. M. J. GREENMAN.

Changes in methods of biological research as pursued by museums and the possibilities of the future: DR. C. E. MCCLUNG, professor of zoology and director of the zoological laboratory at the University of Pennsylvania.

The relation to medicine of institutes of anatomical research: DR. LEWELLYS F. BARKER, professor emeritus of medicine at the Johns Hopkins University.

Luncheon was served at the institute followed by an inspection of the museum and laboratories. In the evening there was a dinner at the Hotel Bellevue-Stratford, at which addresses were made by Professor Charles R. Stockard, of the Cornell Medical School, representing the Society of American Anatomists, and Professor Maynard M. Metcalf, chairman of the Division of Biology and Agriculture of the National Research Council, representing the American Society of Zoologists.

SCIENTIFIC NOTES AND NEWS

THE hundredth anniversary of the birth of Thomas Henry Huxley occurs on May 4. Huxley was born at Ealing and died at Eastbourne on June 29, 1895.

M. PAUL PAINLEVÉ, the mathematician, who succeeds M. Herriot as premier in France, holds the pro-

fessorship of celestial mechanics in the University of Paris.

Dr. Ira Remsen, emeritus president and professor of chemistry in the Johns Hopkins University, has been made an honorary member of the American Chemical Society.

Dr. Leo Loeb, Edward Mallinckrodt professor of pathology at Washington University Medical School, has been elected a foreign member of the Deutsche Zentral Komitee zur Erforschung und Bekämpfung der Krebskrankheit of Berlin.

Dr. Louise Pearce, associate member of the Rockefeller Institute for Medical Research, has been elected a fellow of the Royal Society of Tropical Medicine and Hygiene.

A dinner was given in the honor of Professor Arthur H. Compton, of the University of Chicago, by the Physics Society of Pittsburgh on April 23. Following the dinner, Dr. Compton addressed the members, discussing the scattering of X-rays.

Dr. Joseph C. Bloodgood, surgeon in chief, St. Agnes Hospital, Baltimore, and associate professor of surgery in the Johns Hopkins Medical School, was tendered a dinner by the staff of the hospital on his return from a trip to Egypt.

A reception and luncheon was given by the Chamber of Commerce of Phoenix, Ariz., on April 15, in honor of Dr. William Allen Pusey, president of the American Medical Association, who delivered an address; Dr. Pusey also spoke at a dinner arranged by the Chamber of Commerce at Tucson, and on April 16 addressed the thirty-fourth annual session of the Arizona Medical Association at Bisbee.

At the Geographical Congress in Cairo on April 3 the United States minister presented the explorer Hassanein Bey with the Elisha Kent Kane medal of the Philadelphia Geographical Society.

Sir Francis Younghusband was, on April 2, elected chairman of the Mathematical, Geodetic and Cartographic Section of the International Geographical Congress meeting at Cairo.

At the meeting of the Royal Geographical Society on April 5 the president announced the award of the Royal medals as follows: The Founder's Medal to Brigadier-General the Hon. C. G. Bruce, C.B., for his lifelong geographical work in the exploration of the Himalaya, culminating in his leadership of the Mount Everest Expeditions of 1922 and 1924. The Patron's Medal to Mr. A. F. R. Wollaston for his explorations and journeys in Dutch New Guinea, Central Africa and many other parts of the world. Other awards are: The Murchison grant to Mr. Eric Teichman for his travels in China and Tibet. The Back grant to Captain Bernier for his work in the Canadian Arctic. The Cuthbert Peek grant to Mr. Michael Terry in support of his proposed journey across Northern Australia. The Gill Memorial to Major R. E. Cheesman for his journey to the deserts of Jafura and Jabrin.

Lord Balfour has accepted the presidency of the newly-formed British Institute of Philosophical Studies. Professor L. T. Hobhouse is chairman of the council, and the following are members of the executive committee: The Master of Balliol, Mr. F. C. Bartlett, Professor C. P. Broad, Dr. William Brown, Miss Edgell, Mr. E. Garcke, Dr. Ginsberg, Professor Dawes Hicks, Professor L. T. Hobhouse, Mr. Julian Huxley, Professor F. B. Jevons, Mr. H. T. Laski, Sir Lynden Macassey, Professor J. H. Muirhead, the Hon. Bertrand Russell, Lady Rhondda, Sir Charles Sherrington, Professor Spearman and Miss Stebbing.

C. F. Korstian, associate silviculturist at the Appalachian Forest Experiment Station, Asheville, N. C., has been appointed to a research fellowship in the School of Forestry of Yale University for the coming year, during which time he will be engaged in a study of the factors influencing the germination and early survival of the important American oaks. Both institutions are cooperating in this study.

Professor E. L. Sevringhaus has been granted a leave of absence from the department of physiological chemistry of the University of Wisconsin, that he may spend about six weeks in the laboratory of Dr. Graham Lusk, of the Cornell Medical School. He left for New York early in April and is devoting his time to studies on the respiratory quotient.

Dr. E. W. Adams, professor of chemistry at the Kansas State Teachers College at Pittsburg, has recently joined the research force of the Whiting Refinery of the Standard Oil Company of Indiana.

Paul Jackson, B.S., Kansas, 1923, has become a member of the research department of the Bakelite Corporation.

Dr. Richard Hamer, of the physics department of the University of Pittsburgh, has been appointed chairman of the section of physics of the Pennsylvania Academy of Science.

Wallace E. Richmond, head of the physics department of the Newton High School, has been elected president of the Eastern Association of Physics Teachers.

Dr. Francis R. Fraser, director of the Medical Unit of St. Bartholomew's Hospital, London, is visiting medical schools in the United States.

Dr. H. K. Endell, a ceramic engineer of Germany, specializing in refractories, is visiting the United States. He is accompanied by his associate, Dr. F. W. Steger.

Dr. Albert F. Blakeslee, of the department of genetics of the Carnegie Institution of Washington at Cold Spring Harbor, L. I., has returned from a four months trip in South America undertaken for the purpose of making a study of the Jimson Weed (*Datura Stramonium*). He took part in the third Pan-American Scientific Congress in Lima, Peru, and visited other places on the west and east coasts.

Dr. A. B. Stout returned in April to his work at the New York Botanical Garden after spending two months in southern Florida in a study of the flower behavior of avocados. This investigation was conducted in cooperation with the Florida Avocado Association, the Dade County (Florida) Farm Bureau and the Bureau of Plant Industry, the latter represented in the field work by Mr. E. M. Savage and Mr. T. Ralph Robinson. While in Florida Dr. Stout addressed the Florida Avocado Association on "Flower behavior of avocados with reference to pollination" and he also gave an address on "The clonal variety in horticulture" before the Florida State Horticultural Society at their thirty-eighth annual meeting.

Grettir Algarsson and the men who will accompany him on his exploration trip to the North Pole next month sailed on April 12 from Norway aboard the steamer *Iceland,* for Liverpool, from which port the start will be made. The final dash to the pole is to be made in a small airship of the Blimp type. Mr. Algarsson is taking with him 30 tons of cylinders, containing 50,000 cubic feet of gas for inflating the craft.

Professor H. S. Jennings, of the Johns Hopkins University, gave on April 3 the Phi Beta Kappa address at Goucher College, on "Biology and the advancement of man." He will give, during July and August, a series of lectures at the Hopkins Marine Laboratory, Pacific Grove, California, on "Heredity, environment and development."

On April 8, the annual Alpha Omega Alpha lecture at the University of Illinois College of Medicine was given by Dr. George Dick and Dr. Gladys Dick, of the John McCormick Institute for Infectious Diseases, on the subject of "Scarlet fever."

Dr. William G. MacCallum, Baxley professor of pathology in the School of Medicine of the Johns Hopkins University, delivered the annual Conversational Lecture of the Pathological Society of Philadelphia in the College of Physicians on April 9.

Dr. Harvey W. Wiley, of Washington, D. C., addressed the Philadelphia County Medical Society on April 8 on "The importance of diet to health and vitality."

Dr. Wilhelm Kolle, professor of medicine, University of Frankfurt-am-Main and director of the State Institute of Experimental Therapeutics, addressed the scientific staff of the Rockefeller Institute for Medical Research, New York, on April 17 on "Some new facts in regard to drug fastness."

Professor L. Asher, of the chair of physiology at Berne, has accepted the invitation of four Spanish universities to deliver a course of lectures.

Professor H. A. Lorentz, of Leyden, will deliver the fifteenth annual lecture of the Institute of Metals on May 6. The subject will be "The motion of electricity in metals."

The seventy-fifth anniversary of the foundation of the Royal Meteorological Society will be celebrated on April 21 and 22. The celebrations include a visit to Kew Observatory and a lecture by Professor E. van Everdingen, director of the Royal Netherlands Meteorological Institute and president of the International Meteorological Committee.

A bronze bust of Frederick Courteney Selous, the hunter-naturalist and explorer of South Central Africa, who was killed in action in the campaign in German East Africa, has been placed in the Natural History Museum at Nairobi.

Dr. David Talbot Day, a leading authority on mineral and petroleum deposits, long chief of the division of mining and mineral resources of the United States Geological Survey, died in Washington on April 16 at the age of sixty-five years.

Professor Charles W. Lawrence, head of the engineering department at the University of Southern California, died on April 1 at the age of fifty-three years.

H. E. Jones, past president of the British Institution of Civil Engineers, died on March 24, aged eighty-two years.

Sir William Peck, city astronomer of Edinburgh, in charge of the Calton Hill Observatory since 1889, died on March 7 in his sixtieth year.

Léon Maquenne, distinguished for his work in organic and biological chemistry, has died at the age of seventy-two years.

Nature reports the death of Axel Wirén, professor of zoology at Upsala since 1893.

The Dutch Congress of Natural Science and Medicine was held at Gröningen, from April 14 to 16.

Honorary recognition has been conferred by the University of Wisconsin on four Wisconsin farmers and one Missouri man for outstanding service to the agriculture of the state and nation. Recognition is given by the university every February at the close of Farmers' Week, to from two to five men who have been outstanding leaders in agriculture. Dr. Edward A. Birge, the president, who has just completed 50 years of service in the university, presented the diplomas.

President Coolidge has signed an order establishing national forests in eight military reservations. These are Pines Plains and Camp Upton, New York; Camp Dix, New Jersey; Tobyhanna Artillery Range, Pennsylvania; Camp Meade, Maryland, and Forts Humphreys, Eustis and Lee, Virginia.

UNIVERSITY AND EDUCATIONAL NOTES

The University of Leyden, founded in 1575, celebrated early in April its three hundred and fiftieth anniversary.

Dr. William J. Crozier, professor of zoology at Rutgers University, has accepted a professorship at Harvard University.

Professor S. Lefschetz, of the University of Kansas, has been appointed to an associate professorship of mathematics at Princeton University.

The physics department of Princeton University announces the appointments of the following men as assistant professors: Dr. Henry D. Smyth, former National Research fellow and at present instructor in this department; Dr. Louis A. Turner, National Research fellow at Harvard University; Dr. Allen G. Shenstone, demonstrator in physics at the University of Toronto; Dr. Charles T. Zahn, National Research fellow at Princeton University.

Dr. Gilbert Morgan Smith, of the University of Wisconsin, known for his work on algae, who was a visiting professor at Stanford University last year, will join the Stanford faculty permanently next year. Professor Douglas H. Campbell retires at the end of this year.

Solon Shedd, head of the department of geology at the State College of Washington and state geologist of Washington, has accepted a position at Stanford University, where he will have charge of the John Casper Branner Memorial Geological Library.

Earle B. Phelps has been appointed professor of sanitary science at the College of Physicians and Surgeons, Columbia University. Professor Phelps has for a number of years been a member of the faculty of the department of biology and public health of the Massachusetts Institute of Technology and was later professor of chemistry at the Hygienic Laboratory, U. S. Public Health Service at Washington. The department of sanitary science is the second department of a group which will eventually constitute the Institute of Public Health of Columbia University founded under the terms of the will of the late Joseph De Lamar. The department of public health administration is headed by Professor Haven Emerson, who is responsible for developing this new effort in education and research in preventive medicine. In August the position of assistant professor of epidemiology will be filled and in September a new assistant professor of medicine in industrial hygiene will be added to the staff.

Dr. N. B. Dryer, formerly of University College, London, has accepted an appointment in the department of physiology of Dalhousie University at Halifax.

DISCUSSION AND CORRESPONDENCE

CONFUSING TRAFFIC SIGNALS

A couple of years ago I almost got into serious trouble for flagrant disregard of traffic signals, when, in fact, I supposed I was obeying them, and came to with a queer "gone" feeling when told quite fiercely that I was not. I had wanted to drive across Euclid Avenue, an east and west main thoroughfare, had seen the "stop" signal, had said "That's good" and had driven straight across.

What was wrong? Not only was I tired, but the occurrence took place at the end of a long period in which I had not been driving but had done a good deal of dodging to and fro across the avenue on foot. To a person on the sidewalk the word "stop" in the middle of the crossroads means "It is safe to cross the street you are on" (Figure No. 1); and if most

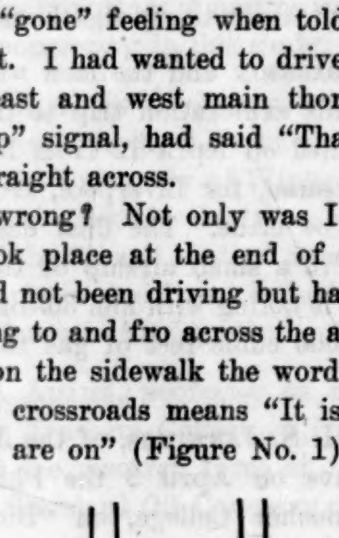

Fig. 1

of a pedestrian's careful street-crossing is done on some one street, like Euclid Avenue, the sign comes

to mean "It is safe to cross Euclid" (or whatever the particular street may be). And then when the pedestrian starts driving again, the red "stop" signal may still mean "Cross Euclid." Stupid, of course, but perfectly natural.

It is a question of confusing a sign and a signal. However loosely the words may be used, they stand for two very different things. A sign is a label that tells something about a thing or place. A signal is a "word of command" to move your body or the car you are driving in a certain way, relative to its present position, but quite regardless of absolute place or absolute direction. A sign tells you more about your surroundings. A signal invites you to forget them. "Gee," "Haw," "March," "Halt," "Right turn," "Left turn," "Full steam ahead" are signals. You can follow them without the remotest idea of where you are or where you are going. Landmarks, buoys, steeples, barber poles, show windows and street names or numbers tacked on a wall are signs. So are such legends as "Entrance," "Exit," "No road," "This way to the Art Museum"; and so is the very appearance of the roadway or the sidewalk. They all give local information and leave you to use it as you will. A pilot or steersman looks for signs; his engineer below looks for signals.

A traffic officer's whistle is partly signal, partly sign. In so far as it means "Go, if you have stopped; stop if you are going," it is a signal. But when, as in Cleveland, one blast means "Traffic will move north and south" and two blasts mean "It will move east and west," it is a sign. A "stop sign" is supposed to be a signal pure and simple.

Now, signs are more natural and more usual guides for either man or beast than signals. It often takes a good bit of tugging at the rein to make the horse you are riding leave the trail. Moreover, the man on the street, whether pedestrian or driver, is surrounded by signs but sees very few signals. He is in an atmosphere of signs, and if he can interpret a signal as still another sign, he tends to do so. And that is what the writer had done when (as pedestrian) he gradually learned to make the red light at the crossroads mean "Euclid Avenue may be crossed."

There are two ways for traffic authorities to avoid this confusion. One (now under experiment at a single corner in Cleveland) is to remove the signal from the middle of the road altogether and put a two-faced light (showing opposite colors at right angles to each other) at each of the four corners over the edge of the sidewalk (Figure No. 2). These lights give the same command to everybody, whether walking or driving: "Approach the blue,[1] but not the red."

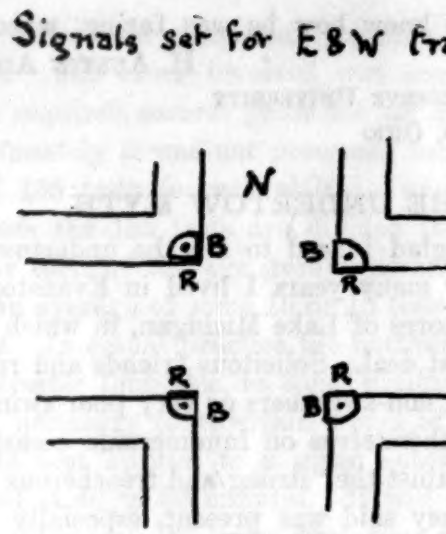

Fig. 2

The other method is to add the informative or sign element of the officer's whistle to the signal element of his lights. Show your colors as at present, but *when traffic is east and west show two lights instead of one,* and make a corresponding change in the appearance of the portable, hand-worked "Go-go tree." The color is the signal, and any stranger can understand it. The number (or shape) is the sign, and the pedestrian who had learned that a single red light or a given red mark means "Euclid may be crossed" would hardly give that meaning to the double red light or the different red mark that he sees from his car when Euclid may not be crossed (Figure No. 3.)

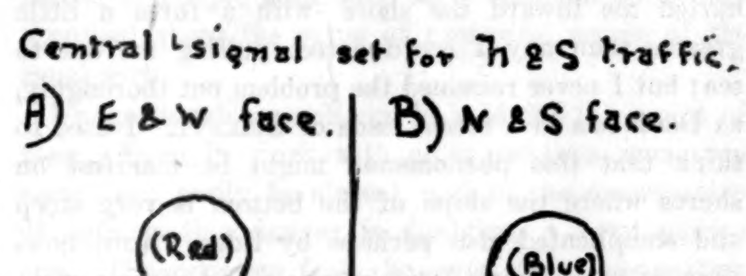

Fig. 3

Incidentally, such a change as this would practically solve the problem of color-blind drivers or color distortion in a fog, for number (or shape) would guide

[1] They actually use green, but blue would make less confusion for the rather large number of red-green color-blind drivers.

a driver, who knew how he was facing, when color could not.

H. Austin Aikins
Western Reserve University
Cleveland, Ohio

THE UNDERTOW MYTH

I am very glad indeed to see the undertow myth exposed. For many years I lived in Evanston, Illinois, on the shores of Lake Michigan, in which I used to swim a great deal. Solicitous friends and relatives (most of them non-swimmers or very poor swimmers) took it upon themselves on innumerable occasions to caution me against the "strong and treacherous undertow" which they said was present, especially during rough weather. Many a time when the waves were dashing very high I have taken my usual swim with but little fear, believing that if I were carried out by this mysterious undertow I would be able to swim up to the surface where, all are agreed, there is no outward flow; for the water could not be towing away from the shore on the bottom and at the surface simultaneously. If this were the case all the water along the shore would recede and pile up in the direction of the center of the lake. After "taking my life in my hands" on many occasions during many summers, I came to the conclusion that, as far as I was concerned, undertow even in stormy weather was nonexistent along the shores of Lake Michigan. In fact, it always seemed to me that the pounding waves hurled me toward the shore with a force a little greater than any I could detect pulling me out to sea; but I never reasoned the problem out thoroughly, as Davis has in a recent issue of Science. I used to think that this phenomenon might be manifest on shores where the slope of the bottom is very steep and complicated also perhaps by tides. Now, however, after studying Davis's article and after reviewing my own experience in rough water, I am convinced that undertow is a myth. Davis's theory that it has arisen from the fear and confusion of people not accustomed to big waves seems to me more than plausible.

As requested by Davis, I will add the following information: I am almost as much at home in deep water as I am on dry land. The slope of the floor of Lake Michigan near the shore is not steep. The direction of the wind, as far as I can recollect, was usually toward the shore. When I used to swim in Lake Michigan I never made a critical study of wave movements at the time. This communication is merely a "memory record." However, I did go into the lake many times in stormy weather at places where an undertow was said to exist and I never was able to detect it.

Walter C. Jones
Birmingham-Southern College

A QUOTATION FROM HIPPOCRATES

To the charge that one has not accorded due honor to Hippocrates, one may not remain silent even if defense is both presumptuous and impossible. In an address (Science, September 5, 1924) use was made of the wording on a tablet on one of the Harvard Medical School buildings. It was of exactly that type of monumental inscription that America is accustomed to receive from Dr. Eliot's pen. Dr. Eliot did, indeed, furnish the particular phrasing used, hence I accepted without question the statement that he had written it.

Several letters from friends calling my attention to the fact that the inscription is a translation from Hippocrates, an anonymous letter on the same subject, and the recent distress of the writer in Science (February 13, 1925, page 184) deepen my chagrin at my ignorance. Had I not been ignorant, however, I could not have made an ultra-modern interpretation of the inscription; an interpretation which many appreciative letters indicate has been helpful.

Boston Francis G. Benedict

"TUMBLING" IN A WILD MOURNING DOVE

Thirty years ago my wife and I observed "tumbling" in a wild mourning dove. A description of the observation was sent to Dr. C. O. Whitman and its receipt acknowledged by him, but, so far as I know, he never made use of the data or made these known. I will give the data here from memory.

We were driving north over flat open country. Mourning doves were flying from the northwest to a "pigeon roost" southeast of us, in a dense apple orchard which had grown up from an abandoned nursery. The tumbling bird was first noticed a sixth of a mile in front of us and to the left of the road. I thought the bird had been shot and hit and I waited for the report of the gun, but none came. The bird balked, fell over backward fluttering, dropped say fifteen feet, recovered and flew on. When no gun report was heard we wondered if the bird might have struck a wire, but saw none in the field where the bird had been. The bird crossed the road an eighth of a mile in front of us and flew diagonally past us at a distance of about a tenth of a mile and disappeared behind and to the right of us. Twice more it tumbled in full view, though at distances of one tenth and one eighth of a mile. Each time the behavior was the same—balking, a fluttering backward fall, recovery while still well above the ground and a renewal of flight. I had not at that time seen tumbler pigeons in flight. When later I did see them their behavior impressed me as similar to that of the wild dove.

Maynard M. Metcalf
National Research Council

THE TEACHING OF EVOLUTION IN NORTH CAROLINA

PERSONS interested in science may be interested to know that Representative Poole introduced the following bill (resolution) for the suppression of the teaching of either the Darwinian or any other evolutionary hypothesis as a fact:

Resolved by the House of Representatives, the Senate concurring, that it is the sense of the General Assembly of North Carolina that it is injurious to the welfare of the people of North Carolina for any official or teacher paid wholly or in part by taxation to teach or permit to be taught as a fact either Darwinism or any other evolutionary hypothesis that links men in blood relationship with any lower form of life.

This bill was referred to the Education Committee which had a hearing and voted to report it unfavorably. A minority report was brought to the floor of the house which was defeated by a vote of 64 to 47.

B. W. WELLS
Z. P. METCALF

NORTH CAROLINA STATE COLLEGE
RALEIGH, N. C.

SCIENTIFIC BOOKS

The Mental Growth of the Child; A Psychological Outline of Normal Development from Birth to the Sixth Year, Including a System of Developmental Diagnosis, BY ARNOLD GESELL, M.D., Ph.D. Macmillan, 1925, pp. 447.

DR. GESELL has accomplished a pioneer and monumental piece of work. The reviewer feels indeed that "The Mental Growth of the Child" is easily the most important book that has ever been written on the early mental and psychical development of children. Its purpose is primarily to serve as a handbook of clinical diagnosis. The procedure involves a combination of test and observational methods. Developmental norms have been established for motor development, language development, adaptive behavior and personal-social behavior. These norms are frankly tentative and sketchy, but they may be used for the assignment of rough developmental age scores to subjects who have been examined.

For motor development there are (if the reviewer has counted correctly) 34 tests; for language development, 24; for adaptive behavior, 58; for personal-social behavior, 39. Many of these are new and most ingenious. All have been applied to 50 children of each of the following age groups: 4 months, 6 months, 9 months, 12 months, 18 months, 2 years, 3 years and 5 years. The subjects were selected so as to secure as nearly as possible representative groups. The labor involved was enormous and of course required several years for its completion.

Fortunately it was not necessary to give the entire list of 155 tests to each child. For use in clinical diagnosis the 155 tests are divided into ten groups, one for each of the age groups named above. This gives an average of some 15 or 16 tests for any given subject. In actual practice the number will be somewhat greater than this, as some preliminary exploration is necessary to ascertain which of the ten series of tests best applies to a given subject. The time required is 40 to 60 minutes. It will doubtless be a surprise to many to know that four-months infants can withstand an examination so prolonged. That they can do so is due to the simplicity and informality of the tests.

The author does not intend that his method shall be used as a psychometric tool, in the strict sense. Quantitative scores are not given on the separate parts of the examination, but simply an A, B or C to indicate roughly the quality of the performance. The assignment of such qualitative scores is frankly a subjective procedure. Usually the author sums up the results of an examination in terms of a "developmental age," although it is not made clear just how the individual qualitative scores are summed. The author repeatedly insists that it must not be made a matter of simple addition. As a clinician he is very skeptical about the value of numerical scores of the usual sort.

Unquestionably much can be said for this point of view. Even in work with older subjects numerical scores can easily be abused, and in the examination of infants this danger is doubtless a very serious one. The reviewer feels, however, that the position which the author has taken on this point detracts from the value of his methods for both practical and scientific use. If a developmental age score is to be assigned at all, it ought to be the most accurate one that the data at hand make possible. Scores in terms of "A," "B" or "C" are certain to be given widely different meanings by equally competent examiners. Uniformity of procedure in giving the tests is also rendered difficult in some cases by lack of explicitness in the directions. No attempt is made to establish the reliability of the various tests or their exact diagnostic significance (validity). Statistical results are not given; in a majority of cases not even the percentages of children "passing" a given test.

To the reviewer it seems that in steering so clear of psychometric technique, the author has sacrificed much of the value of his data. For example, the

child's drawing of a man is rated only as A or B, although Dr. Goodenough has recently demonstrated[1] that by the use of a refined scoring technique this test, used with unselected children of four or five years, can be made to yield a correlation of .70 with Stanford Binet mental ages. Something like this may be true of many of the other tests.

Probably the greatest technological weakness of medical diagnosis is the play it gives to the subjectivity of judgment. Dr. Gesell's methods will be, or at least should be, widely used by physicians; it is unfortunate that his treatment of the subject does not set the medical practitioner a better example of objectivity. Detailed standardization and statistical evaluation will of course be carried out by others, and the danger is that it will be done by investigators who lack the extraordinarily rich clinical experience which Dr. Gesell has had.

But the reviewer has already devoted too much space to what he regards as a defect. If defect it be—and one may admit that the question is debatable—it is heavily overshadowed by the merits of the work as a whole. The author has staked out new territory of great promise in one of the richest of the border-line fields between psychology and medicine. His treatment is original and suggestive in highest degree. The clinical comparisons between typical subjects of adjacent age groups are little short of dramatic. The fifteen or more brief chapters devoted to the larger aspects of early mental diagnosis are admirable in substance and form. The reviewer predicts that the book will bring about a renaissance of interest in observational and experimental work with young children. It ought to have a wide distribution not only among physicians and psychologists, but also among parents and teachers. Its delightful literary style, its freedom from technical jargon and its wealth of illustrations (there are 227 figures) make it a book for every intelligent person who is not too stupid or too unimaginative to take an interest in this most dramatic and fascinating of all the "ages of man."

Lewis M. Terman

Stanford University

Antics of the Ants and *Ingenious Insects*. Two volumes bound in one. By Alfred Mark Salyer, Glendale, California. New York, Laplante and Dunklin.

This superbly printed volume may be described as a rollicking book of science. Following quotations from leading entomologists, it is adorned by humorous cartoons, and conclusions of students of insects are expanded into verse, often reminiscent of Mother Goose and at other times rising almost to seriousness.

While the wisdom of the ants and their genius for cooperation is highly extolled, our author quizzically warns us against other insects, with whom "a desperate war" must be waged, in which "man's civilization, his very life, are at stake," and the victorious insect "hosts will be swarming throughout the smouldering ruins of his last handiwork."

David Starr Jordan

Stanford University

The Romance of the Holes in Bread. A Plea for Recognition of the Scientific Laboratory as the Testing Place for Truth. By I. K. Russell, member American Chemical Society; Author of "Hidden Heroes of the Rockies," and "Frontier Tales of the Townsend House." Easton, Pennsylvania, The Chemical Publishing Company.

A modest but thoroughly wholesome bit of popular science is Isaac Russell's "Romance of the Holes in Bread." Beginning with the common and apparently commonplace process of bread-baking, Mr. Russell takes up the nature and functions of the yeast-plant, and from it the general qualities of bacteria, incidentally at the same time describing the obstacles encountered by men who in earlier ages realized that we know nothing whatever of the universe, save that which through the ages we have found out, not thought out, to be true.

All this leads to a record of the life and work of Louis Pasteur, greatest of Frenchmen and one of the noblest figures of all time. Phases of the work of Pasteur and of his disciples are indicated graphically in chapter headings: "Finding the source of plagues"; "From ferments to sanitation"; "Bake oven to bandages"; "From poultry to vaccination"; "The conquest of hydrophobia"; "Back to the bakery."

David Starr Jordan

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A FIELD TRIP AID

During the writer's observation of birds and nests he has found the contrivance described and sketched below to be an invaluable addition to the equipment (see Fig. 1).

This small bit of apparatus consists of an ordinary circular pocket mirror two and one half inches in diameter, around which is twisted about twenty inches of eighteen gauge copper wire. The wire is put on in such a manner that by bending it at a right angle to the plane of the mirror and by bending the lower loop backward parallel to the mirror, an ordinary

[1] In a doctor's dissertation not yet published.

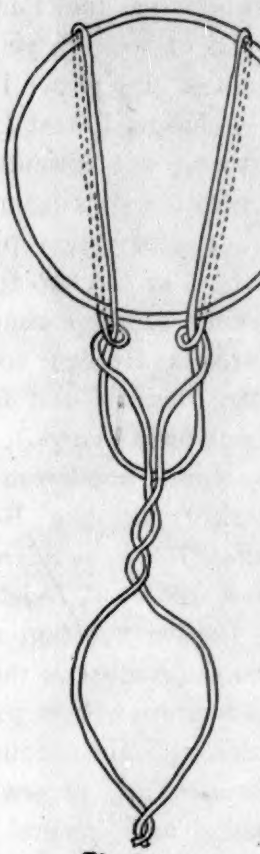

Fig. I.

stick can be pushed up through both loops. With the mirror placed thus, one is able to see the contents of many nests which are out of reach and which could not be looked into without the aid of a ladder or support of some kind. It is especially valuable to observe contents of nests which are located in bushes or small saplings six to ten feet from the ground. It also may save the observer a useless climb into a larger tree.

By bending the wire in the same general plane as the mirror, it can be conveniently carried in the pocket or handbag.

Sticks of varying lengths are always available in the woods and the writer has made use of a broom on several occasions where the nests were located in trees near a house.

This device is also quite valuable when there are a number of people in the party, a bird class, for instance, as it gives all a chance to see the eggs, young or empty nest practically at the same time, and thus disturb the nest for but a short time.

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SPECIAL ARTICLES

THE RELATION OF EXERCISE TO RICKETS IN WHITE RATS[1]

IT has been observed by Paton, Findlay and Watson in their experiments on dogs that those kept in the country and freely exercised in the open air, although they had actually a smaller amount of milk fat than those kept in the laboratory, remained free of rickets, while those kept in the laboratory became rachitic. This supported their belief that diet alone was not the sole factor in producing rickets—that exercise and sunlight were both instrumental in the prevention of rickets. It was on the basis of such observations that this experiment was started to determine, if possible, whether exercise (as well as sunlight) tends to prevent rickets.

Litter No. 123 (from a mother kept away from ultraviolet light and fed a diet low in antirachitic vitamine), consisting of eight albino rats (four females and four males), were placed on a diet March 14 which in previous experiments had proved to be rickets-producing, with the exception of one rat. This rat was given a diet containing "red dog" flour high in phosphorus, thus serving as a control since such a diet would normally not produce rickets.

The apparatus consisted of a series of individual wire-netting tread-mills of the squirrel cage form, the end pieces of which have a diameter of about 16 inches and are mounted on a two-tier rack. Each cage is equipped with a counter so that the number of revolutions made by the animal is registered. Each cage is equipped with a receptacle for food or water.

Diet No. 341, low in phosphorus, and containing relatively much calcium, was made according to the following formula:

NaCl	2 per cent.
Plaster of Paris	2 " "
Lactalbumin	8 " "
Yeast	1 " "
Flour (high patent)	87 " "

The phosphorus content was determined to be about 138 mg per 100 gms of food. This diet is also lacking in antirachitic vitamine and vitamine A. During the last week, one gram alfalfa meal daily was given each rat to cure xerophthalmia which had developed. Rat VIII was fed on the same diet with the substitution of red dog flour as a control.

Each animal was kept well supplied with the diet and distilled water; weighings were taken every week, and note was made of abnormalities, such as sore eyes. Rats Nos. I, II and III were given the greatest amount of exercise for two hours between the hours of 5 and 7 P.M. Rats Nos. IV, V and VI received one hour of the same exercise. Rats VII and VIII received no special exercise at all. At the end of a week, rats Nos. I, II and III were exercised from 8 P.M. to 7:30 A.M. (except Sunday), and an average of 1,000 revolutions per night was quite constantly maintained. This would equal a distance of

[1] From the Laboratory of Physiological Chemistry, University of Minnesota.

a little less than a mile per night. For the rats receiving the least exercise (IV, V and VI) periods in the revolving cages were given on alternate nights. This method was in accordance with the work of Slonaker who found that the period of activity for the albino rat was during the night between 8 P.M. and 3 or 4 A.M. and the period of rest during the day. The experiment was continued over a period of six weeks beginning March 14 and ending April 25. At this time the animals were X-rayed and autopsied.

At autopsy the following findings were obtained:

THOSE RECEIVING MOST EXERCISES

Rat I Costochondral junctions enlarged decidedly in middle four ribs, larger on right than left.

Rat II Costochondral junctions decidedly enlarged lower half of both sides.

Rat III Costochondral joints noticeably, but not as markedly enlarged as I and II; more so on the right lower six ribs.

THOSE RECEIVING NO EXERCISE

Rat IV Costochondral junctions very slightly enlarged, more distinct on left lower five.

Rat V Costochondral joints slightly enlarged.

THOSE RECEIVING NO EXERCISE

Rat VII Showed slight nodules on right lower six costochondral junctions; smaller on left side.

Rat VIII Died April 2, no rickets.

X-ray plate showed mild rickets in all rats except VIII.

TABULATED RESULTS

	Body Weight in Grams					
Rat. No. Date	3/14	3/21	3/29	4/4	4/11	4/18
I	27	32	35	37.5	42	46.5
II	25.5	32	36	41	42	47
III	26	32	35	41	44	45
IV	23	30	37	42	43.5	40
V	22	27	31	32	35	37
VI	23.5	28	30	30	30	30
VII	26	32	35	40.5	44	42
VIII	21	38	42	Died 4/2		

Conclusion: Exercise does not prevent rickets.

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COINCIDENCE BETWEEN THE RANGES OF FORMS OF WESTERN YELLOW PINE, BARK BEETLES AND MISTLETOE

IN 1924 the writer[1] called attention to some hitherto obscure differences between the Pacific Coast and Rocky Mountain forms of western yellow pine. Following the publication of this paper Dr. F. C. Craighead, entomologist in charge, Forest Insect Investigations, U. S. Department of Agriculture, has called the writer's attention to the striking manner in which the ranges of two species of yellow pine bark beetles correspond to the ranges of the two forms of western yellow pine. The range of the mountain pine beetle (*Dendroctonus monticola* Hopk.) corresponds very closely to that of the Pacific Coast form of western yellow pine (*Pinus ponderosa* Laws.), while the Black Hills beetle (*Dendroctonus ponderosa* Hopk.) ranges over the same territory as the Rocky Mountain form of yellow pine (*Pinus ponderosa scopulorum* Engelm.) These two species of *Dendroctonus* do not overlap, which still further substantiates the writer's contentions as to the differences in the characteristics of the two forms of western yellow pine. Dr. Craighead states that although the adults of these two species of bark beetles are somewhat difficult to separate, their habits and general bionomics are strikingly different.

A comparison of the ranges of two species of western yellow pine mistletoes with the ranges of the two forms of yellow pine also reveals a striking coincidence. Hedgcock[2] records *Razoumofskya campylopoda* (Engelm.) Piper as occurring very commonly on *Pinus ponderosa* in the Pacific Coast region and *R. cryptopoda* (Engelm.) Coville, the eastern form, as "very common on the western yellow pines, *Pinus ponderosa* and *Pinus ponderosa scopulorum,* in the states in the region of the Great Basin and Rocky Mountains from eastern Washington to [Arizona and] New Mexico." Thus it is seen that, with the exception of a slight overlapping in the northwest, each of these species of mistletoe attacks a particular form of the western yellow pine.

These two parallel cases tend strongly to strengthen the belief that biologic forms change readily in response to the environment. The environmental conditions which characterize the habitats of these two forms of western yellow pine and the associated bark beetles and mistletoes are sufficiently diverse to result in climatic variants of these species. Many so-called species of plants and animals differ from each other because of their differing reactions to the same or to widely diverse environmental conditions.

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[1] Korstian, C. F., "A silvical comparison of the Pacific Coast and Rocky Mountain forms of western yellow pine," *American Journal of Botany*, 11: 318–324. 1924.

[2] Hedgcock, George G., "Notes on some diseases of trees in our national forests. V." *Phytopathology*, 5: 175–181. 1915.